si négligées," he has omitted all Capt. Jacob's measures subsequent to 1848, and as instances where some measures are wanting, may be mentioned γ Argûs, ρ Herculis, δ Herculis, τ Ophiuchi, 70 Ophiuchi, ε Equulei 61 Cygni, θ Indi, &c., &c.

After exhibiting the measures of each object, M. Flammarion, in the great majority of cases, appends his own conclusions with respect to the cause of the relative changes of position, which have generally been carefully considered, though there are some few in which we should hardly be disposed to follow him. But the reader having nearly all that is known of the different objects before him, in M. Flammarion's summary, will be able to form his own inferences. If an observer he will be guided thereby to a selection of objects most worthy of his attention, or most requiring further measures for the elucidation of the cause of altered position.

In a provisional examination of the volume ample proof is afforded of the care taken by the author in his work, which has no doubt been as he describes it long and laborious. There are a few such oversights as H₃ 3678 for H₂ 4087 ; and under Procyon, misled by a measure of Secchi's in 1856 as printed, he refers to a companion at 83°·6 and 33″·16 ; this measure, however, really belonged to Powell's distant companion, and instead of 33″·16 the distance should be 331″·6, as it is given in another page of the same volume of Memoirs of the Roman Observatory. There is no reference to some of Argelander's determinations of proper motion, as in the case of a distant companion of γ Leonis, upon which M. Flammarion enters into some detail. Omissions like this, however, are perhaps unavoidable in the first preparation of such a work, but the author will doubtless have his attention called to them, and will be able to make his second edition a still more inclusive manual of double-star astronomy, than even this first impression.

Through the kindness of Leverrier, M. Flammarion was allowed the use of one of the equatorials at the Observatory of Paris during the year 1877 for the re-measurement of a number of the double stars ; these measures applying to about 130 objects are given at the end of the preface to this volume : amongst them we note the close pair of 40 Eridani, a rapidly revolving star which has not received the attention it deserves from observers.

M. Flammarion's work will doubtless soon find its way into the hands of every one who is interested in the double and multiple stars, and he will certainly experience the satisfaction of receiving the well-earned thanks of many amateurs who have no convenient access to large astronomical libraries and to whom his volume will be a valuable *vade-mecum*.

OUR BOOK SHELF

The Mollusca of the Firth of Clyde; being a Catalogue of Recent Marine Species Found in that Estuary. By Alfred Brown. (Glasgow : Hugh Hopkins, 1878.)

ALTHOUGH the recent mollusca of this district have during the last few years received a good deal of attention, especially from the labours of M'Andrew, Barlee, and Merle Norman, still the various memoirs detailing the results of these labours were only to be found widely scattered through a number of scientific periodicals, and Mr. Brown has in this neatly printed work given us not

only a *résumé* of the labours of the naturalists we have referred to, but also of all those who have collected on the Firth of Clyde, and joined these to the labours of Mr. David Robertson and his own. The result is, so far as the testaceous mollusca go, a large and apparently accurate catalogue, which will show not only what has been done but also among the nudibranchs and cuttle-fish what is yet to be done. The notes under the heading of Habitat in this catalogue are often most interesting, giving details not only of the exact localities for the species, but notes also of their local names.

Wanderings in Patagonia; or, Life among the Ostrich Hunters. By Julius Beerbohm. Map and Illustrations. (London : Chatto and Windus, 1879.)

THE title of [this book is somewhat misleading, as the author's "Wanderings" were of a very limited extent, embracing only a small portion of the south-east coast region of Patagonia. Its most important feature is the account given of the life of the ostrich-hunters, and it adds little to our knowledge of Patagonia in addition to what has been told us by Musters and the one or two others who have really "explored" more or less of the wild region. The author's story is pleasant to read.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Tempel's Comet

THE well-known comet- and nebula-finder of the observatory of Arcetri, Tempel, has just made an observation of great interest in reference to his Comet No. II. of 1873, which, as astronomers know, has an orbit between Earth and Jupiter. It has no tail, but a nebular surrounding, which Tempel observed to be gradually diminishing in luminousness without losing bulk, and finally has entirely disappeared, leaving the comet perfectly distinct, but with a slight scintillation or rather an appearance of being composed of several masses having motion in the rest of the nucleus ; probably an optical effect due to our own atmosphere, but which is at all events seen quite distinctly enough to make it certain that the disappearance of the nebulous surrounding is not due to failure of the telescope to show it.

The comet was last observed on December 18, at 6h. 53m. 12s. mean time of Arcetri in Right Ascension 23h. 3m. 14·15s., and in South Declination 19° 15′ 54″·8. It was seen on the 21st but briefly, and no observation could be made. Since then the continually cloudy sky has prevented it from being seen, but Tempel is confident of being able to see it through January. It is now amongst the asteroids.　　　　W. J. STILLMAN

Florence, January 1

The Cosine Galvanometer

IN NATURE, vol. xix. p. 98, my name appears in a way that might lead the reader to infer that I was the inventor of the "cosine galvanometer." My knowledge of this useful instrument was derived from Prof. Trowbridge, of Cambridge, U.S., who described it in 1871 in the *American Journal of Science*, vol. cii. p. 118. In my "Physical Manipulation" I omitted to mention Prof. Trowbridge's name, supposing that his connection with the instrument was too well known to render it necessary.

　　　　　　　　　　　　　　　EDWARD C. PICKERING

Harvard College Observatory, Cambridge, U.S., December 20, 1878

Force and Energy [1]
II.

IN passing it may be noticed that the plus sign thus deduced for a tensive force is otherwise convenient because tension results in a positive increase of the dimensions in the direction of the tension of the body through which the tension is transmitted.

[1] Continued from p. 196.

With regard now to the investigation of the equilibrium or the acceleration of momentum of a body founded on a know-ledge of the forces acting upon it, that is, of the various rates of transference of momentum between it and other bodies through its surfaces, we must evidently give signs to the various surfaces of the body, surfaces which are parallel, and face opposite ways, being given opposite signs. If we multiply the transferences of momentum taking place through the different surfaces each by the sign of the surface through which it takes place, and add all these products together, the sum will be the acceleration of mo-mentum of the body. Thus two equal tensive forces acting through parallel opposite faces would keep the body in balance. Or two numerically equal forces, the one tensive and the other compressive, acting through parallel surfaces facing the same way, would also keep the body in balance. If the phrase "intensity of force" be used to mean the force per unit area through any surface, so that it is simply a generalisation of the two common phrases "intensity of pressure" and "intensity of tension;" then if each small element of the surface of a body be given its proper sign and multiplied by the intensity of force through that small element, this force being also given its proper sign, and if all such products for the whole surface be summed up, the total will be the acceleration of momentum of the body. The direction of this acceleration will be shown by the sign of this total, the sign having reference to the relative position of the surfaces which have been arbitrarily called positive and nega-tive. Thus let two tensions, *i.e.*, positive forces, be applied to two parallel opposite faces, and let the force applied to the posi-tive face be greater than that applied to the negative face ; then the body will suffer a positive acceleration of momentum ; that is, an acceleration in the direction from the negative face towards the positive face. The faces perpendicular to the positive and negative faces must be given the signs $+ \sqrt{-1}$ and $- \sqrt{-1}$.

Thus a pair of positive forces applied to faces with the signs $+ 1$ and $+ \sqrt{-1}$ cannot possibly balance each other. But a positive force applied to a $+$ face can be balanced by a tangential, or shearing, force applied to a $\sqrt{-1}$ face. Be-cause the shearing force has either the sign $+ \sqrt{-1}$ or $- \sqrt{-1}$, and multiplied by the sign of the face, gives either $+ 1$ or $- 1$, as the sign of the product. Surfaces oblique to what is chosen as the positive direction must be considered as partly scalar and partly vector, as also forces oblique to the sur-faces through which they act, or rather oblique to their direction of transmission. Oblique surfaces must be multiplied by oblique forces according to the ordinary rule of vector multiplication. This system of notation requires no further explanation, I think, to those who are likely to approve of it.

It has become lately a common habit to look upon those things which are conserved, that is, those which have an enduring existence, as objectively real ; while those which may come into existence and go out of it again are considered as objectively unreal. Whether this is a correct philosophic habit or not, it has certainly tended to create suspicion as to the objective reality of all mechanical quantities. A gradually extending recognition of the relativity of these quantities is apt to lead on to a reluctant apprehension that all so-called physical facts are mere formal logical deductions from arbitrary definitions. The dark shadow of distrust first fell upon momentum because the fact that it is distinctly a relative quantity is most easily recognised, and thus became earlier a part and parcel of our familiar ideas. Then somebody suddenly recalled to mind the distinction, according to definition, between external and internal kinetic energy, and found that the external kinetic energy which it had been fondly hoped had some lingering flavour of the ABSOLUTE still clinging to it, was no more than a part of the internal kinetic energy of a larger group of bodies ; and it became clear at a glance that energy, that grand ABSOLUTE REALITY which, being once borne into existence by triumphant modern science is now far too carefully conserved by its enthusiastic worshippers to allow of there being any risk of its dropping again out of existence, is just as purely relative in its nature as the velocity which has to be squared in order to calculate its amount. It had been thought that because a velocity has a direction and the square of a velocity has no direction, therefore we might calmly and fearlessly contemplate the total or partial destruction of momentum, steadfast in the assurance that energy would still live for us. And thus with much waiting in fluttering hope and trembling fear upon the brink of the Unseen Universe, and becoming impatient at the non-arrival of any clear intimations of immortality for ourselves, or for energy, or even for matter itself—which is clearly neither more real nor more unreal than her faithful spouse energy—a cloud of dismal despair seemed to be settling on the heads of the scientific nations, when a stern but cheering voice was heard from Munich bidding us be satisfied with our finite human faculty of perceiving relations only, and promising us that, if we would only not aspire to divine knowledge of the absolute, we might KNOW even now and also hereafter.

While admitting fully the relativity of all the physical facts which we may learn, I think it would be very unfortunate if we were to allow ourselves to confuse this with the idea that all mechanics is a mere phantasmagoria conjured up by a process of formal logical deduction from a basis of abitrary defini-tions. The clearest exponent of this theory of formality in mechanics that has come to my notice is Dr. V. A. Julius, in his letters on "Time" to NATURE, vol. xvi. pp. 122, 420. The argument may be thrown into the form of four short pro-positions and a conclusion, all of which are derived by purely formal reasoning from the ordinary definitions of the various quantities involved, and which a friend of mine pretends make out a "clear demonstration of the utter absurdity, futility, and falsity of all mechanics."

1. All motions and velocities are simply relative. Within a given isolated system, nothing with reference to the motions of its parts can be known beyond the motions of these parts relatively to the centre of inertia of the system.

2. Relatively to any other system, or single body, the velocity of the centre of inertia of this first system is, by definition, simply the mean of the velocities of its parts. The sum of the velocities of the parts relatively to the centre of inertia of this first system is, therefore, always zero.

3. Within one portion of this system, therefore, there cannot be any loss of average velocity without there being a simul-taneous equal gain of average velocity in some other portion.

4. The changes which can possibly take place in the system with regard to velocity consist, therefore, in balanced exchanges of relative momentum between its parts, and, therefore, the equality of "action and reaction"—whether calculated with reference to rate of transference of momentum, or with re-ference to rate of transference of energy, *i.e.*, rate of doing work—is a purely formal deduction from the definition of the centre of inertia.

Conclusion. A purely formal deduction from an arbitrary definition is just as likely not to agree with reality as to agree with it. Q.E.D.

The fallacy of the argument lies in the artful omission of a few words in 3, which are necessary to make the meaning quite explicit. At the end of 2, the sum set equal to zero is that of the velocities *relatively to the centre of inertia of the system* itself. These, therefore, are the velocities referred to in 3. Therefore, in 4, the exchanges of momentum that are balanced are those of momentum measured relatively to the centre of inertia of the system itself ; and it does not at all follow, by *pure logic*, that such a balanced exchange of momentum rela-tively to this centre does not produce an acceleration of velocity of the centre of inertia relatively to some body outside this system. Of course, if we add this outside body to the first system, then pure logic will compel the exchanges of momentum throughout this new combined system and measured relatively to the centre of inertia of the new combined system to balance. But pure logic does not *necessitate* the exchange of momentum within one part of the system relative to the centre of inertia of that part being unaccompanied by a simultaneous exchange of momentum between that part and some other part, or every other part. Thus the fact of conservation of momentum is not, that when two bodies exchange momentum, the amounts lost and gained measured relatively to the centre of inertia of the two, are numerically equal,—that would be a mere truism—but that the amounts lost and gained measured relatively to a third body are equal to each other. This latter is a physical fact, only to be proved by experiment, not by logic. The statement that action and reaction between two bodies are equal, does not mean any-thing in particular ; but the statement that the action of a force between two bodies does not accelerate the velocity of their centre of inertia relatively to a third body is a statement of experimental fact. The mechanics of a system of two bodies might be built up by means of formal reasoning alone ; but not that of a system of three, or of more, bodies without the experi-mental establishment of the law of conservation of momentum.

And the more complicated the system be, the larger the number of possible combinations of three bodies within it, the greater is the number of experiments or observations we can make to prove that the conservation of momentum is a general physical fact. The larger the number of such observations becomes, the further removed is the doctrine of the conservation of momentum from the character of a logical deduction from definitions.

Still, of course, the doctrine has only to do with relative velocities and relative accelerations of velocities. It loses, however, none of its reality and truthfulness on account of this. Why should not relations be capable of being real, even if not permanent? We are indeed incapable of conceiving anything as real which does not owe its reality in our conception simply to its relations to other things. If objective reality is in any way the opposite of relativity, then, certainly, so far as our knowledge goes, there is no such thing as objective reality. Our notions of momentum and of force, then, are relative to three bodies, and not to two bodies, and this seems to me to be an important point. The ELEMENTARY notion of momentum derived from DEFINITION is relative to TWO bodies only; but the PRACTICAL notion derived from EXPERIENCE is relative to three bodies at least, or to a complicated system of bodies. It should not be forgotten that the physical realities among which we live owe their existence to the complexity of nature. Throughout the complexity there are certain simple invariable relations, and these are the physical laws of nature. The law of conservation of momentum is this : the momentum of one system relative to another system remains unchanged by exchanges of momentum between the parts of the former system. Otherwise stated it is : exchanges of momentum may and do take place between the parts of a system without these exchanges being necessarily accompanied by an exchange of momentum between this system and any other system.

Energy is, of course, a quantity of as relative a character as momentum, although its relativity is not of just the same kind. Energy in general is usually defined as the power of doing work. Curiously enough this definition is frequently followed closely by the statement that a system may possess a very large amount of energy, and yet if there are no differences of potential within it no work can be done by it. The correct statement of what is meant by this last has often been given, viz., that in this case no work can be done by one part of the system upon another part of the same system. But still more often is the inaccuracy indulged in of saying that energy of one kind or another may be transformed into work. Now work is not energy and has no kind of similarity to energy, and therefore energy can never be converted into work. When energy is transferred from one body to another the first does work upon the second, the amount of work done being measured by the amount of energy transferred. The rate at which energy is transferred is the rate of doing work, or the horse-power. The doing of work or more shortly WORK, is the transference of energy from one body to another, but is not the energy itself. The confusion has never entered into the practical use of the word " work," which has always really been applied in the sense here explained, although very probably a good deal of confusion of ideas among both practical and theoretical men, may have been caused by the above noted incorrect statement that energy and work are convertible. The confusion is of the same sort as if we were to use the word force in the sense I have advocated and confuse it with acceleration of momentum. During some transferences of energy there is an invariable transformation of energy. If during the transference, the whole of the energy transferred is also simultaneously transformed, then the rate of doing work is also equal to the rate of transformation, and the amount of work done is numerically equal to the amount of energy transformed. But the phrase " work done " is only used when transference takes place. When a portion of one kind of energy in a body is converted into energy of another kind without any energy leaving the body, it is not the custom to say that work has been done. Work is only done by one body upon another, so that work is the TRANSFERENCE, not the TRANSFORMATION of energy. To say that so much energy has been spent in doing an equivalent amount of work is a convenient and quite allowable mode of saying that this amount of energy has been transferred from the working body without specifying what has become of the energy ; that is, without specifying into what other body the energy has been transferred, and without specifying in what form the energy has appeared in the other body. But to say that the energy is converted into work is quite a different thing, and altogether wrong.

When a body possesses in two parts of it two quantities of heat at two different temperatures, the amount of work which the one part has the power of doing on the other in consequence of this difference of temperature is not nearly equal to the whole amount of heat energy in the two parts. Thus the energy in a body is not the power measured quantitatively, possessed by its parts of doing work on each other.

If in a collection of bodies there be a certain one body with a certain amount of kinetic energy, calculated from its velocity, relative to the centre of inertia of the group, that one body might deliver up the whole of this kinetic energy by direct impact upon another body which had zero velocity relative to that centre of inertia, provided these two bodies were exactly alike in certain particulars as to mass and shape. But if there did not exist in the group any body which had this particular relation of shape and velocity to the first, then this first could not possibly deliver up all its kinetic energy, so as to get its velocity relative to the centre of inertia of the whole group reduced to zero. It is thus clear that the internal kinetic energy of a collection of masses is not measured by the amount of kinetic energy calculated from the velocities relative to the centre of inertia of the collection that can be transferred from one part to another.

Also, if another body, or another group of bodies, existed apart from this first group, and possessed a velocity of centre of inertia either zero, or of any other value, relative to the centre of inertia of the first group, the kinetic energy of this first group, measured either relatively to its own centre of inertia, or to that of the other group, or to the centre of inertia of the two combined, could only be wholly transferred to this second group, provided that this second group had very special and very ingeniously contrived relations with regard to mass and configuration to the first group. Thus the kinetic energy of any collection is not measured by the power it may possibly have of doing work upon bodies outside the collection. And quite evidently the same may be said of any other kind of energy possessed by the body.

For each kind of energy we have more or less accurate means of comparing quantitatively different amounts of that kind of energy, and thus of measuring the amount of that kind of energy possessed by a body in terms of the quantity which is adopted as unit of that kind of energy. We have also means of converting different amounts of any one kind into most other kinds of energy ; and since in several carefully-made experiments upon the conversion of different kinds of energy there has on the whole been a very fair agreement in the ratios furnished by these experiments between the adopted units of the different kinds, we have come to believe in the truth of the law of conservation of energy—the more especially since this belief is supported by theoretical reasoning based on the hypothesis of the truth of the conservation of momentum. This latter theoretical reasoning, however, we have, hitherto, at any rate succeeded in applying only to transferences of kinetic energy of visible motion, and to the thermodynamics of perfect gases.

But taking this principle of conservation of energy for granted as true, we have the means of measuring the amount of energy of any kind possessed by a body in terms of the adopted unit for kinetic energy of visible motion.

<div align="right">ROBERT H. SMITH</div>

<div align="center">(*To be continued.*)</div>

The Unseen Universe—Paradoxical Philosophy

WILL you permit me to ask through your columns how the idea of the authors—that the present universe is developed out of our unseen universe, which unseen universe is itself developed out of another, and so on in an endless vista up to the unconditioned—works when applied to the present universe as itself developing a lower universe ?

The present universe must be a conditioning as well as a conditioned universe, or there would be a breach of the principle of continuity, and there must, on the same principle, be an endless vista of such lower universes.

Have we any hint of any lower universe ? Ought we not to have more than a hint ? Ought we not to be fully conscious that our own universe is developing and sustaining such a lower universe, to the living intelligent beings in which we are, in fact, supernatural agents, as the angels in the universe above us are to ourselves ?